REVIEWS.

ART. XV.—*Anæsthetics: their Position and Prospects.*

Deutsche Chirurgie; herausgegeben von Prof. Dr. BILLROTH in Wien, und Prof. Dr. LUECKE in Strassburg. Lieferung 20. Dr. O. KAPPELER: *Anæsthetica;* mit 18 Holzschnitten, 105 Curven in Zinkographie und 3 Lithograph. Tafeln. Stuttgart, 1880.

Anæsthetics. By Dr. O. KAPPELER; being Part XX. of "*German Surgery*," conducted by Profs. BILLROTH and LUECKE.

Three Reports on the Action of Anæsthetics to the Scientific Grants Committee of the British Medical Association. British Medical Journal, Jan. 4 and 24, and June 21, 1879.

The Advantages and Accidents of Artificial Anæsthesia: A Manual of Anæsthetic Agents. By LAURENCE TURNBULL, M.D., Ph.G., Aural Surgeon to Jefferson Medical College Hospital, etc. 2d ed., revised and enlarged. Philadelphia: Lindsay & Blakiston, 1879.

THE most appropriate time for the consideration of a scientific subject is when some great advance has just been made, which places it in a changed and new position, or when doctrines have been, by long study and experience, so firmly established that they meet with general acceptance, and promise, at least for the near future, something approaching stability. It is needless to say that the present is not such a period in the history of anæsthetics. On the contrary, we find that in regard to one, chloroform, confidence is most thoroughly shaken, the practical disadvantages of ether are daily felt and lamented, while the frequent resort to various combinations of these, and the new agents which are being brought forward to supplant the others, are evidence of the unsettled state of professional opinion and the unsatisfactory condition of the subject.

But widely different as is the position of the subject from what we would desire, some good may result from a general consideration of it. Possibly, extreme views may be moderated, conflicting opinions disposed to harmonize, or, if not, it is something to look over past progress and see just where we are standing, what is the outlook ahead, and try to make preparation for possible further advance.

The appearance of the books whose titles are placed at the head of this article affords us an opportunity to make such an attempt. Dr. Kappeler's deserves the first place from its completeness, its accuracy, and its thorough scientific character. It will be noted that no title is attached to the author's name, and nothing indicates an official position. But his qualification is guaranteed by the work being one of a series, which, as a whole, is to present German surgery by the best German surgeons. In the list of authors we find such names as Billroth, Breisky, and Bruns, Esmarch, Hildebrandt, Nussbaum, Thiersch, Volkmann, Winckel, and many others equally well known. The student will not proceed very far in the book before he will discover that its author needs no indorsement from any one. His actual experience with chloroform is stated at "about 5000 administrations," but the careful study devoted to the subject is shown throughout, and especially in the sphygmographic tracing taken during anæsthesia, even during the early stages, a period presenting great difficulties, and in the careful noting of all the physiological facts attending every step of the process. Dividing the subject into general and local anæsthesia, he has treated it after the methodical and exhaustive manner of his countrymen. The bibliography occupies nearly seven pages, and includes much of the journal literature of this country and of Great Britain. In regard to the chemistry of anæsthetics, the physiology of anæsthesia, practical experience with other different compounds, and full presentation of all information upon the subject, the work must be considered complete up to the date of its publication.

The reports of the committee noted at the head of this article, brief as they are, form one of the most valuable contributions of recent times to the study of anæsthesia and anæsthetics. We could not expect less than work of high scientific character and practical value from a committee[1] appointed by the British Medical Association to investigate so important a subject. It is true that the ground gone over is not large in amount, but it has been thoroughly worked, and, as a consequence, has yielded well. The committee followed two lines of inquiry: first, to discover wherein lies the special danger of chloroform; second, to try if some anæsthetic agent could be found which would avoid these dangers. Distinct subjects of study were: the changes produced in the gases of the blood, the changes in the gases of respiration, and the effect of anæsthetics on the nervous condition and on mental phenomena in man. These investigations have been made by experiment upon animals, with the most approved instruments of precision, and with careful observation of every occurrence. The manner of experiment is important. The trachea of the animal was opened, and respiration carried on artificially, at the same time the sternum was removed, and the heart thus submitted to visual observation, while the blood pressure in the arteries was marked by the sphygmograph. Thus anæsthetic vapours could be added to the air inspired without any irritant effect upon the upper air passages, thus the different effect of different vapours upon the circulation was apparent, and in some instances was striking, and thus respiration was separated from the circulation and the effect of the vapour upon each of these great functions could be observed and studied while carried on independently of the other.

We regret that we cannot speak of Dr. Turnbull's work in terms of equal praise. It is a book having in many respects great merit, but having also most serious blemishes, faults, and errors, which the appearance of this, the second edition, afforded an opportunity of removing and correcting. The author has succeeded very well in attaining the objects in view, as stated in the preface of the first edition, of presenting the description, the chemistry and chemical tests, and modes of administering anæsthetics, together with his personal experience with them and the mortality resulting from their use. He has produced a treatise containing a large amount of valuable information which the student and practitioner will find difficulty in procuring elsewhere, but this information is not methodically presented or well arranged for either pleasant reading or profitable study. Were this all, there would be but little ground for complaint, but, unfortunately, the book contains serious errors and careless statements, and upon some points there is shown a spirit of unfairness, which must seriously interfere with its claims to be considered a scientific treatise. Some of the blemishes would not exist, and some of these serious charges could not have been made, had there been more careful proof reading. It is not satisfactory to read of Budin and Coyne's studies of the pupils in anæsthesia as studies of the pulse, and we imagine that Dr. Culbertson, of Ohio, although he may overlook the misspelling of his name, will not relish the presentation of his prize essay as the "*Sin* of Anæsthesia in Midwifery" instead of the "use"! Again, such statements as those on pp. 208, 224 should have attention called to them because positively dangerous! One is that chloral hydrate should be given with great care [!] to little children in doses of *three grains for each month!* The other, that a friend of the author has used "with success," in the obstinate vomiting of pregnancy, chloral in two-drachm doses, repeated in an hour or two if necessary! It may be inferred that the administration is to be by enema, but it is not so said, and even then, the dose stated is far too large without a word of caution being added. We regret to find, and to be obliged to call attention to these disagreeable features in a book which, with a little more care and attention, might have been a treatise of high character.

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[1] The committee consists of Joseph Coats, M.D., Pathologist, Glasgow; William Ramsay, Ph.D., Assistant to Prof. of Chemistry; and John G. McKendrick, Prof. of Physiology, University of Glasgow.

The two leading anæsthetics have experienced, since their introduction, wide variations of fortune. Chloroform, the discovery of which followed so closely upon the use of ether for anæsthesia, received a hearty and enthusiastic welcome. Its power, its certainty, the rapidity of its action, its convenience for the surgeon, and its lack of unpleasant characters for the patient insured its reception everywhere as a substitute for ether, and almost banished the latter from practice. But the dark feature of chloroform, its danger, very soon made its appearance, and able and influential advocates demanded the reinstatement of ether to its former place in professional favour. In two cities, Boston and Lyons, the change was immediate and thorough. In the former, where chloroform had never, probably, obtained the footing it had elsewhere, its use was entirely abandoned by 1853. In the latter, Diday and Petrequin opened the campaign in favour of ether as early as 1849, prompted by some fatal accidents from chloroform. The force and persistence with which the attack was carried on by the Lyonnaise, and the fact that Paris and other cities continued to adhere to chloroform, made the two anæsthetics subject of more polemical discussion, and of greater diversity of opinion in France than in any other country. Sedillot, of Strassburg, entered the field with the dogmatic proposition that "pure chloroform, well administered, never kills," and under the leadership of Malgaigne, the Paris Academy of Medicine declared, but not without dissenting voices, that with the observance of certain precautions, chloroform was entirely harmless. Velpeau, however, testified before the courts that chloroform might cause death, even when administered with the greatest caution. Yet the Surgical Society of Paris declared, in 1859, that ether could not take the place of chloroform because its narcosis was stormy, and entire muscular relaxation was not caused by it, unless pushed to a stage in which life was in danger. This brought from the medical Society of Lyons a counterblast; ether was declared to be innocuous, and its narcosis as perfect and as deep as that of chloroform. In 1866–7 Petrequin renewed the war against chloroform, and urged the fact that during fourteen years' exclusive use of ether in Lyons no death had occurred. In reply, Velpeau advanced his large personal experience with chloroform without an accident, and Sedillot again entered the field, his motto now modified, however: "the use of chloroform is not without danger, but it may be avoided by care, experience, and skill." In 1867 Laroyenne, of Lyons, published a fatal case of etherization, and six other cases were collected by Gayet, and although they would not all bear critical examination, they served to show that ether was an agent not altogether devoid of danger; the strength of attack of the Lyonnaise school was broken, and in France ether has never succeeded in supplanting chloroform.

In Great Britain, chloroform was, for the first decade and a half after its discovery, almost the sole anæsthetic used; ether had a few adherents, men of high character and standing. Edinburgh especially led in devotion to her native anæsthetic, and, like Sedillot's terse statement, that chloroform "*ne tue jamais*," the sententious doctrine of Syme and Lister, that "a case for operation is a case for chloroform," had a wonderful influence, as all such dogmatisms have. Soon after 1870, however, the tide began to turn in favour of ether. The change of current is attributed by Dr. Kappeler, to whom we are indebted for the facts of this historical sketch of the subject, to three facts: First, the increasing number of deaths from chloroform, and the enterprise of the British journals in placing these unfortunate cases before the profession, even to their minutest particulars. Second, the influence of Dr. Joy Jeffries, of Boston, at the Ophthalmological Congress at London, in 1872, in demonstrating the prompt effects of ether, in showing a simple method of administering it, and his impressive appeal in favour of it. Third, the demonstration of the Chloroform Committee, by experiment upon animals, of the minor degree of danger from ether. And now we are told it has the preference in many of the metropolitan hospitals, although chloroform is by no means abolished, while its use is extending through the country, and at the north especially, the mighty influence of Keith, with his splendid ovariotomy record, is powerfully felt in its favour.

In Germany chloroform has held a less disputed sway than in any other country, and is now, Dr. Kappeler says, so far as he knows, exclusively used. In Austria, however, the course pursued by Billroth is an index of the lack of full satisfaction with either chloroform or ether. He is an advocate of, and constantly uses, a mixture consisting of three parts of chloroform, one part of ether, and one part of absolute alcohol.

In regard to this country it is difficult to make any general and absolute statement of the past or the present, which being true of some sections would not be untrue as to the whole. Dr. Kappeler contents himself with stating that ether has never been entirely laid aside, but has always shared, even outside of Boston, professional favour. He gives as authority the statistics of the civil war: chloroform administrations, sixty per cent.; ether, thirty per cent.; mixtures of the two, ten per cent. Also those of the Marine Hospital [Service?] of the United States for 1874: chloroform, fifty-two per cent.; ether, thirty-five per cent.; mixtures, thirteen per cent. Of course in Boston, and throughout the territory where her influence is felt, which is certainly all of New England, ether has reigned supreme. We do not believe, however, that the statistics given above correctly represent the relative administrations of the two agents throughout the rest of the country by the profession at large. In our opinion chloroform was almost exclusively used, outside of New England, up to the few past years. Within ten years the current has been setting more and more strongly in favour of ether; the number of deaths from chloroform have become so numerous that they could not be overlooked; many have occurred under circumstances which afforded no plea as an escape; so that its dangers have been forced upon the attention of the profession, until now there is a feeling of distrust and fear which approaches to a general belief in the criminality of its employment except under special circumstances. Probably an exception must be again made in regard to this statement as to the South, that part which formed the Confederacy. There we believe chloroform to be much more generally employed and more freely resorted to than anywhere else in this country. We base our opinion upon the able and enthusiastic championship of this anæsthetic by Dr. Chisholm, of Baltimore, the only printed, as it is the warmest, advocacy of chloroform which has come to our notice for many years;[1] also upon the statement of Dr. Landon B. Edwards, given in Dr. Turnbull's work (pp. 110–111). This confidence in chloroform seems to be based upon the experience of surgeons with it in the Confederate army, and at the North we have found the warmest advocates of this anæsthetic among men who had large surgical experience during the late war.

We will give Dr. Kappeler's general statement of the present position of the subject in England, which will indicate also his opinion of the relative merits of the two anæsthetics and the direction in which he anticipates further knowledge.

> "The re-introduction of ether in England had as consequence a very considerable and important improvement in the art of administering it. It is undeniably more skilfully given, its effects more rapidly produced, and it is used with less discomfort and unpleasantness to the patient; yet in spite of all this ether has not the general usefulness and reliability of action, the simplicity of administration, the insignificance of after-effects of chloroform, and only the proof, based upon the broadest foundation of clinical experience, that ether narcosis was *considerably* less dangerous than chloroform narcosis, can give the victory to ether. This proof, however, has as yet been by no means furnished. Since the introduction of ether to England there has been recorded a considerable number of deaths from it, besides those formerly known, and the fatal cases have been where necessary precautions had not been neglected and such that the cause of death must with justice be attributed to the ether. It will be shown, farther on, how difficult it is to decide as to the relative danger of different anæsthetics, to attain to a correct percentage of the mortality from chloroform and from ether, yet it is probable that if in England, the country most favourably situated by the number and concentration of its medical institutions, the advanced standing of its medical societies, and the careful attention which from the beginning has been devoted to this subject, the deaths from ether shall be published with the same conscientiousness with which the deaths from chloroform have heretofore been made known, the materials will soon be obtained for the decision of a question so important for medical science as well as for suffering humanity."

Death under anæsthetics is of course the great and important fact connected with their use. The event is so shocking and painful to all concerned, the reverse is so dark where, without this, all is so brilliant, that its occurrence has from the first excited the deepest interest and stimulated the most scrutinizing investigation and closest study. It is safe to say, that it will continue to do so until we shall have attained the solid basis of assured knowledge. The deaths from chloroform seem to have much increased in frequency of late years, and now amount to a very considerable number. Dr. Kappeler says that "about 300" have been published, but is certain that many are unknown. He knows of four in his vicinity which were never published, and in this connection again takes occasion to compare the English with German journals, very unfavourably to the latter. Dr. Turnbull gives a table of 160 cases which have occurred from 1869 to 1879, and these, in addition to the table of the former edition of his book, make a total of 370 cases. But if the actual number of deaths cannot be ascertained, much less can we arrive at the relative number as compared with the number of administrations, a piece of information which would be of the greatest interest and most important value. Every effort at computing the total number of inhalations as compared with the number of deaths has been in the highest degree unsatisfactory and but the merest guesswork. Where statistics are reliable there is shown the widest variation in the proportion. Many of the deaths occurred of course to individuals who had administered the agent comparatively but a few times. In one hospital there was one death in 525 administrations, while on the other hand there is Syme with 5000 cases and no death; Nussbaum and Billroth in Germany, the first with 15,000 cases, the second with 12,500, and no deaths; Dr. M'Guire in this country, 15,000 cases and one death; the French army in the Crimea, 20,000 cases and no death; our army in the civil war, one death to 11,448 administrations. The computations of Dr. Andrews and Dr. Coles, of this country, are both given by Dr. Kappeler, the former making one death to 2723 cases, the latter one to 2873. Richardson's estimate is one death to every 2500 administrations, but his figures, made up of over 35,000 cases recorded in English hospitals, show one death to 3196. These may serve as indications and approximations; where two factors of the problem are not and cannot be known, it is useless to look for an accurate third. Whatever may be the mortality from chloroform, either absolute or relative, there are some points which cannot be left unconsidered without injustice. Thus, all cases should be deducted in which the administrations were by an unqualified person, or where the plainest rules of safety were violated. Thus in Dr. Turnbull's table of 160 deaths, in eight the administration was by a dentist or chemist, and two were cases of self-administration, = six per cent. In a collection of 133[1] cases there were seven cases of self-administration. Dr. Kappeler enters the plea that after all the figures, and admitting that chloroform is an element of danger in and of itself, yet it is at least doubtful if the cases of sudden death during operation caused by it exceed those which took place from fright and terror before it was known. He might have said even more than this. In Edinburgh, at an early time in the history of chloroform, when its administrations had amounted to many thousands without accident, there had occurred during the same period two sudden deaths on the operating table of this character.[2]

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[1] What anæsthetic shall we use? By Julian J. Chisholm, M.D., Baltimore, 1877.

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[1] Am. Jour. Med. Sci., Oct. 1867.

[2] Sir J. Y. Simpson's Works, Anæsthetics, p. 146.

We cannot imagine how Dr. Turnbull should attribute to the writer of this the view that death in the earliest stages is caused *only* by emotion (p. 96), as we have only maintained that to be one of the causes of death. He takes occasion to say that he does not agree with this view, "else the same result would have followed when ether or nitrous oxide is employed." Well—turning to p. 201 of his work, where is recorded the death from nitrous oxide reported by Dr. Newburgh, of New York, we read that "the inference seems entirely justified, that death was not caused by nitrous

But the cause, or rather the physiological process of death under chloroform is a subject of far deeper interest than a computation of numbers. The importance of the inquiry is at once apparent, for the *modus operandi* of the deleterious influence once discovered, the way to avoid the peril would be plain. So, to account for these deaths, we have had explanations without number, hypotheses the most untenable, and theories the most fantastical; ingenuity has been taxed to the utmost in the formation of some of them, and logic torn to tatters. This may seem to be exaggeration, but the facts warrant every word, as a brief examination will show. Thus we have so high an authority as Mr. Lister[1] maintaining that the greater number of deaths from chloroform arose from an "overdose from too long-continued administration;" and this in face of figures showing that nearly fifty per cent. of them took place before narcosis was complete, and a large number during the very earliest stages.

The theory of a peculiar idiosyncrasy on the part of the patient, originating with Robert, has found lately an adherent in Billroth.[2] Yet we find no such idiosyncrasies in regard to other medicines; somewhat greater effects may be produced in some patients than others, but opium, strychnia, or arsenic never cause death when administered in ordinary doses. But admitting the most extreme claims for the existence of idiosyncrasy, in order to accept it as a satisfactory explanation of death from chloroform we must admit that it is as changeable as the wind. A large number of those who have died under chloroform had taken it before, some of them several times; then we must assume that a patient may have to-day a constitution under which a medicine is safe, and to-morrow such an one as renders it fatal.

An explanation of death was found for a time in impure chloroform, but this theory had but a short life, and we then had theories of chemical and of mechanical effects. According to some of the former, oxygenation of the blood was interfered with in various modes, or the vital fluid was directly acted upon and changed, and by the latter it was now a mechanical closure of the larynx, and again an expansion of the anæsthetic in the nervous centres producing death by compression of the delicate structures!

The mechanical theory of death from a collection of mucus in the larynx and air passages was soon shown to be false by performing tracheotomy on animals and by failure to find the alleged cause *post mortem* in a single case. That in regard to obstruction by falling backwards of the tongue is yet in vogue. But the knowledge of the possibility of this accident furnishes a reliable safeguard, and, as Dr. Kappeler well remarks, in all the later deaths there has been no sin of omission upon this point in a single case, yet, nevertheless, the deaths occurred.

Some of the theories have been more plausible, more reasonable, and, because accounting well for a fair proportion of the cases, have enjoyed for a time the favour of many, both leaders and led in the profession. Of these the doctrine that the cause of death lies in faulty administration has been the longest lived, for it began with Sedillot's "le chloroforme pur et *bien employé* ne tue jamais;" and is yet in vogue with some. It accounted, and to our mind satisfactorily, for those cases which elude all other explation, where death takes place in the very earliest stages. The sudden and violent impression upon the terminal branches of the par vagum produced,

oxide gas," but it is "entirely probable that the cause of death was *nervous shock* from *dread of pain and apprehension* of fatal effect from the inhalation of an anæsthetic agent"!

[1] Holmes's Surgery, vol. v. p. 488.

[2] Kappeler, p. 110.

by reflex action, sudden cessation of the heart's action. Such a view is supported by analogy, is reasonable, clinical experience accords with it, and it is, Dr. Kappeler says, the teaching of all late physiological experiments. Nothing sustains this view more strongly than the report of the Anæsthetic Committee as to the variation in blood-pressure in rabbits produced by the addition of more chloroform, the animal being partially anæsthetized :—

"When the animal is not fully under chloroform, *any fresh administration causes most remarkable variations in blood-pressure,* with retardation of the heart's contractions."

No one can critically examine the record of chloroform deaths without being struck with the number in which the fatal event followed immediately upon the addition to the sponge or apparatus of a fresh supply of the anæsthetic agent.

The theory of faulty administration as the sole cause of death from chloroform is a peculiarly seductive one, especially to those who have had large experience without meeting with a misfortune. Some special mode of administration, or some measure of precaution, like the preliminary dose of whiskey, is seized upon as an explanation of the immunity from accident, and then the work of convincing others, that "if my plan be followed there is no danger," is entered upon with enthusiasm and positiveness! We have felt in the past all the force of this doctrine, because fully convinced that many deaths have been caused by mal-administration, because this doctrine gave the best ground for the continued use of an agent that we did not wish to abandon.

But the theory is no more tenable than the others. To assume it to be correct we must first conclude that the chloroform was badly administered in all those cases of death in hospitals, where it was in the hands of men who had made it the subject of most careful study, who had had large experience, and who devoted to its use the most scrupulous care. We must conclude that Snow did not know how it should be given, that Simpson himself, after introducing it, and after using it for nearly thirty years, did not know how to administer it. As Dr. Kappeler remarks, a speedy end has been brought to many a dream of fancied security and belief in the harmlessness of chloroform.

Next comes the theory that death is caused by the inhalation of too concentrated chloroform vapour. This theory is sustained by all the points which explain the early deaths as already given, and also in the most marked manner by experiment upon animals, which show, too, that it is not alone in the early stages that death is thus caused. This was the theory of Snow, and his influence was powerful in causing its reception, especially in his own country, where it has always been the favourite theory. It "runs like a scarlet thread," to use the expressive language of Dr. Kappeler, "through all the chloroform literature of England." To this theory the existence of inhalers is largely due. Experimenters had agreed very closely in the results obtained, and had fixed very nearly the due proportions of air and chloroform vapour for safe administration. Thus Snow gave five volumes of vapour to ninety-five of air, and showed plainly that ten per cent. was dangerous. Lallemand, Perrin, and Duroy found that animals lived a long while in a four per cent. mixture of chloroform vapour, but died rapidly in one of eight per cent. The English Chloroform Committee stated the maximum for safety to be four and a half per cent. As it was a simple problem of mechanics to construct a machine regulating the amount of vapour inhaled, and the due proportion of air, the vital problem seemed equally easy of solution. Yet what has been the result? The history of inhalers for chloroform administration is full of disaster. Of thirty fatal cases, carefully selected as bearing close scrutiny in regard to every precaution having been used, the administration was by an inhaler ten times. Anstie had dangerous symptoms once in one hundred and forty-five times, and used an inhaler in two-thirds of his cases. Of ninety-five fatal cases reported by the Chloroform Committee, an inhaler was used in twenty-eight. The best device, and indeed a perfect instrument, for insuring a due and known mixture of chloroform vapour and air is Clover's rubber bag, into which is introduced a measured quantity of the anæsthetic, and breathing from which by no possibility can the patient get an undue proportion. Yet of fifty-six fatal cases tabulated by Dr. Kappeler five took chloroform from Clover's bag, and in four other of these cases an inhaler was used. Moreover, this source of danger has been recognized by the profession long enough. Sansom, following in the footsteps of Snow, emphasized it. It has neither been overlooked or forgotten, and still deaths from chloroform have rather increased than diminished.

Diversity of opinion as to the mode in which death under chloroform takes place has been equally marked. There have not been quite as many different doctrines and explanations, on account of the limited number of vital organs of the body to which they could be attached. The blood, the nervous centres, the heart, and the lungs, have each severally been looked upon as the special seat of action of the deleterious influence. Asphyxia, syncope, paralysis of the heart, interference with the functions of the nervous centres of circulation and of respiration, destruction of the corpuscles of the blood, and of the nervous cells by chemical action of the inhaled vapour, have all had their advocates, and no one of them has been satisfactory to or accepted by all. It would seem to a layman, perhaps, that there could be no excuse for this diversity of opinion on so momentous a subject, and that order would be speedily brought out of confusion by the results of *post-mortem* examinations. Yet this generally reliable resource fails here. Examination of the bodies of those who have died under chloroform does not furnish any uniform change or condition of any of the vital organs. Fluid blood, the one single universal condition, belongs to sudden death without reference to the cause. The lungs have been sometimes congested and sometimes anæmic; and the brain in various states as to the amount of blood in its vessels. Marked changes are generally found in the heart, but they are not uniform; it is sometimes distended and sometimes contracted, although in the majority of instances the right cavities are loaded with blood. Many of the deaths being distinctly cardiac in character, a determined effort has been made to connect them with a pathological condition of the structure of the heart, fatty degeneration of its muscular fibre, and this condition is looked upon throughout the profession, we believe, as one of peculiar peril for the administration of chloroform. "Fatty degeneration of the heart" is readily accepted as a sufficient explanation of the fatal result. Dr. Kappeler does not share in this view, denies, indeed, that it has any sound clinical foundation, and since the point is an important one we will give his reasons. In his table of one hundred and one cases of death the results of *post-mortem* examination are fully given in fifty-three. In the report of these examinations fatty condition of the heart is mentioned more or less explicitly sixteen times, yet only five times was it based upon a microscopic examination. He then says :—

> "But it must not be forgotten that in these examinations the charge of fatty heart is made in order to relieve chloroform and its administration, and that, therefore, to a minimum amount of degeneration an importance is ascribed which in reality it has not and which would not bear a critical investigation.
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> "I cannot avoid stating here what my friend, Prof. Roth, of Basle, a careful and conscientious observer, wrote upon this point: 'Minor degrees of fatty degeneration of the heart I meet with in the sections I make, with very great frequency, under the most various circumstances, in advanced age, in acute and chronic diseases, etc., and I do not rely upon ocular but upon microscopic examination.'
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> "That to fatty heart is to be ascribed only a relative and by no means an absolute danger, is apparent from the fact that of my sixteen patients with this complication eight had already before, and mostly but a short time before, inhaled chloroform, and in view of the great danger that is ascribed to this pathological condition it has always appeared surprising to me that I have never met with an unpleasant symptom in chloroforming for twenty-six amputations on account of senile gangrene, in which, besides the affection of the arteries, in four cases wide-spread fatty degeneration of the cardiac muscle existed as shown by *post-mortem* examination and microscopic investigation. Also, I failed to find in the literature of the subject a single case of death from chloroform when it was given during the existence of senile gangrene, a condition which is notorious for frequency of association with disease of the muscular structure of the heart. I remember Virchow's words: 'Of very great importance is the condition of the cardiac muscle, which especially in senile mortification is *very frequently atrophied and the subject of fatty degeneration.*'"

A careful examination of all the facts will show, we believe, that the heart has received too great a share of attention as the starting point of disaster from chloroform, and that the importance of the respiration in this respect has been overlooked. Of all the vital functions this is the most immediately affected by inhalation of chloroform, and upon this there is a twofold action, the direct influence of the vapour upon the air-passages and an indirect influence through the respiratory nervous centre. Of all the vital actions respiration is the most variable under the administration, passing through all degrees of irregularity and even to complete cessation of considerable duration from tetanic muscular contraction. In animals it is the function which constantly succumbs before the circulation. A critical examination of reported cases of death sustains this view. Thus in Dr. Kappeler's list, and we give this alone because it is the latest contribution to the subject, and time will not permit full presentation of all cases, there are forty cases in which the symptoms preceding death were well described and carefully recorded. In seventeen of these the danger was first announced by the respiration, and this function ceased before the action of the heart stopped. Of these cases two groups may be formed, the first in which narcosis was perfect, ten cases; a second, in which full anæsthesia had not been reached, seven cases; in all the latter death took place during great muscular excitement or rigidity. Where the facts are fully known it appears, therefore, that in over forty per cent. the death was not cardiac, but directly or indirectly respiratory.

Death by a direct action on the nervous centres, of which the cardiac and respiratory symptoms were the expression, was probably too simple a theory to be popular. Explanation of this action by surmising a dissolution of the fat of the nervous molecules, a deleterious influence upon the protagon, and similar hypotheses were plenty. But the commission of the *Société Médicale d'Emulation* reached the plain truth. It denied that the sudden deaths were caused either by asphyxia or by cardiac paralysis, but rather by a "cessation of the function of the nervous centres, which absorb the anæsthetic vapour, their excito-motor power is destroyed, respiration ceases, then the circulation, and death results."

Accumulated facts and their study brought finally the only conclusion possible. Among all the conflicting opinions as to modes of death and different theories as to its causes, the majority contained more or less of truth, some were absolutely correct, yet no one of them would suffice to cover the whole ground. In other words, the causes of death are various and the modes of death are different. Chassaignac and Maisonneuve recognized a death by syncope and by asphyxia, and they were followed by Weber who amplified upon them. Then came Sansom's excellent work teaching that death may be by syncope, by asphyxia, and by necræmia.[1] Later, in 1870, Richardson gave four classes, death by syncopal apnœa, by epileptiform syncope, by cardiac paralysis, and death by depression of the nervous system. Erichsen also recognized three modes of death: by asphyxia, by coma, and by syncope. To this class of authors Dr. Kappeler belongs, and we may give his views as presenting the result of the latest and a most careful study of the subject. At the outset he makes the statement that "in spite of the large amount of physiological study as to the action of chloroform, which has been successful in substituting much knowledge for hypothesis, and in spite of careful clinical observation, it is not yet possible to give a satisfactory explanation of all deaths from chloroform." It is indubitably proved, he says, that there are a large number of the deaths from syncope, cardiac, and also a large proportion from asphyxia, the respiratory actions ceasing before the circulatory. Referring to his table of the deaths by syncope, seven took place before complete narcotism was produced, some of them at the very outset of the inhalation. A portion of these he says undoubtedly belong to that class of well-known sudden deaths on the operating table before chloroform was discovered. We would suggest, also, that the administration of the anæsthetic itself, introduces a new, a mysterious, and a powerful emotional influence quite as likely to cause death in this manner, as fear of the operation. A great dread of the anæsthetic has been a striking feature in some of these cases, and, as seen in a case already referred to, not confined alone to chloroform. As accounting for other of these deaths, he fully accepts two explanations for which we have always contended: first, a death from shock, the primal impression being the surgical operation, the patient being only partially anæsthetized, producing by reflex action cessation of the heart's movements; and second, one in which the impression is upon the air passages by the sudden inhalation of concentrated chloroform vapour. The syncopal deaths occurring under the full operation of the agent he explains by syncope, a syncope which is the direct effect of the chloroform. It is not that chloroform, by its depressing influence, converts a syncope originating in other causes into a fatal one, but "the syncope itself is produced by the chloroform." The deaths by asphyxia he also divides for study into those occurring while narcosis is incomplete and those when it is perfect. In the former the dangerous symptoms have always been preceded by great muscular excitement and contraction, and whatever explanation may be made of them the favouring influence of the tetanic contraction of the chest muscles and its retarding influence upon the respiratory process, must not be overlooked. For asphyxia in deep narcotism the direct influence of the agent upon the respiratory nervous centre is to be considered. For these and the cardiac cases, whatever may be proposed or suggested, possible or probable, "we are fully justified in assuming a sooner or later appearing paralysis of the centres of respiration and circulation situated in the medulla oblongata."

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[1] Dr. Kappeler refers to and quotes an article published in the number of this Journal for Oct. 1867, but why he has added a class of "cases affected with chronic heart disease," which is not to be found there, it would be difficult to explain. He does not seem to recognize that Class VI. of that article illustrates and sustains his own conclusion, announced a few pages further on, that some cases are entirely inexplicable.

A careful, a thorough, an exhaustive study of the action of chloroform, and of the accidents from it, leads inevitably and unmistakably to but one conclusion. With the single exception of the suddenness with which danger appears, there is nothing regular but irregularity, nothing certain but uncertainty. Neither the direction from which disaster may come nor time of its occurrence is to be relied upon. Even when the administration has ceased, the point of safety has not been reached. In Dr. Kappeler's fatal case, two minutes had elapsed since any chloroform had been inhaled, and this is not a single instance. It may be objected that possibly there is a mistake about this and some other points, that the circumstances are not favourable for accurate observation. Then we will refer to the experiments made by the British Medical Association Committee, the accuracy and carefulness of which place the seal of demonstration upon this feature of the action of chloroform. We quote from the report, italicizing some of the words and sentences.

> "Chloroform may cause death in dogs by primarily paralyzing either the heart or the respiration. The variations in this respect *seem* to depend to some extent on individual peculiarities of the animals.
>
> "Chloroform has sometimes an *unexpected* and apparently [?] *capricious effect on the heart's action*, the pressure being reduced with great rapidity almost to *nil*, while the pulsations are greatly retarded or even stopped. The occurrence of the *sudden and unlooked-for* effects on the heart's action seems to be a source of serious danger, all the more that in two instances *they occurred more than a minute after chloroform had ceased to be administered and after the recovery of the blood-pressure.*"

It is this irregularity and uncertainty of action which has shaken confidence in chloroform, and the demonstration of this stamps it as a *treacherous* remedy, and shows that no measures of precaution can prevent, no care avert disaster. Velpeau, at an early period of the history of chloroform, discerned this, and made his declaration to the French courts, that it might cause death in spite of all precautions. Richardson, after a careful study of the subject, said, "that no human skill in applying it can divest it of its dangers;" and now comes Dr. Kappeler to repeat, almost word for word, their opinion as the result of his study and experience. It is the conviction of this uncertainty and unreliability which has turned the current of professional opinion away from chloroform; which has caused hundreds to abandon its use, not willingly, but sadly, in obedience to the stern logic of facts. We can appreciate and realize the feelings under which they have acted. Honouring chloroform for the wide extension it has made to the bounds of operative surgery, esteeming it as the chief handmaid of their art, cherishing it for the untold human suffering it has abolished, prizing it for the certainty, the celerity, and the pleasantness of its action, they have yet felt compelled to give it up. They have parted with it not without some of that emotion which, it is said, should not enter into science; they have parted from it as they would bid farewell to an old friend, prolonging the last grasp of the hand, delaying to speak the final word, sick at heart with the feeling that but for one fault there could be no other such friend as this.

Yet chloroform is not going to pass into oblivion, or even into entire disuse. There are two classes of cases in which, in all human probability, it will still be used, because the benefits resulting from it are immense, and the dangers to be feared from it as nearly as possible at zero. As an anæsthetic for children chloroform stands yet unrivalled; and when a better one is found the profession will truly have cause for congratulation. Deaths have occurred from it, in this class of subjects, it is true, but so rarely that we believe it entirely justifiable to use it, and even repeat it daily for a painful dressing or examination. Dr. Kappeler's table contains two cases of death of children—one two, the other four and a half years of age; Dr. Turnbull collects eighteen cases as having occurred since 1849. No numerical estimate of the proportion can be made, yet clinical experience has fully shown that the amount of danger to this class of subjects is infinitesimal. Even the last-mentioned admits the use of this anæsthetic for "very young children."

Women in labour constitute the other class of cases for which chloroform bids fair to be yet long used. As an assuager of the pains of parturition it has proved itself efficient, reliable, without danger, and with only disadvantages and drawbacks that can be avoided by such careful and judicious use as should always attend the administration of potent remedies. So well recognized are its advantages, not merely as an anæsthetic for surgical obstetrics, but as an agent for the diminution of suffering, that the use of it in natural labour is no longer defended or apologized for in our text-books, but directed and, under proper restrictions, encouraged. Of all the gifts of medicine for the alleviation of the suffering of woman, this is the brightest and the best.

Entertaining these views, we can feel but little satisfaction with Dr. Turnbull's treatment of this part of the subject. He grudgingly admits that the experience of British practitioners "is generally understood to have disposed them to regard the use of chloroform in labour as a proceeding of the highest utility and moment and practically free from danger." This is all he can say in its favour. He alludes to France, where chloroform in labour has "not yet become acclimated." It is true that some honoured names in the roll of French obstetrics have opposed its use for the relief of pain.[1] Yet the extent of their experience may be judged of from the fact that they have taken the utterly untenable ground that there is no such thing as demi-anæsthesia, even ridiculed its existence. On the other hand, there have been able advocates for its use even in France, and the dissenters have been but a fraction of the profession.

Dr. Turnbull copies the conclusions of a paper read by M. Richard before the International Medical Congress of Geneva, the first of which reads: "The employment of anæsthetics is, as a general rule, advisable in natural labour," with the remark that "they are *not generally approved of* by the majority of gynæcologists." [?] He should have named the dissenters and given their opinions. He quotes from Dr. Lusk the conclusions of a paper read by that gentleman before the American Gynæcological Society on "The necessity of caution in the use of chloroform during labour," and fails to copy the statements made in the body of the paper that the administration of chloroform during natural labour is legitimate, or the admission that there is not on record a well-authenticated case of death from it during labour when administered by a qualified medical man. Having read this, as he must have done, he gives as an instance of death from chloroform during labour, without any comment, a case in which the patient had had a convulsion, for which the anæsthetic was administered, and just before she died she had another "slight tremor." [Convulsion?] He gives again, without comment, a case in which "*three pints of chloroform*" were used as an instance of its injurious effects! All this is not fair, and it is upon this that we base a charge of lack of the scientific spirit as a feature of Dr. Turnbull's book.

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[1] It would be interesting to know how much *personal* feeling has had to do in this matter. It would not be the first time that such feelings had influenced the progress of science. The late Charles James Campbell, of English birth, won for himself in the French capital a brilliant reputation, a most enviable position, "with immense practice, and crowds of wealthy and influential patients from all countries." He was an ardent advocate of chloroform in natural labour, and his success was largely due to his use of it. This, naturally enough, would not be very pleasant to native Parisian obstetricians, and conservatism and personal feeling might be easily confounded. See Dr. Playfair's Annual Address to the Obstetrical Society of London.

It is not an easy task to arrive at satisfactory conclusions in regard to ether, or to state fairly its relative results. We are not here embarrassed by the number and variety of theories as to its action, for the difficulty is rather a paucity than an excess of literature. It is a singular fact, in view of the rapid advance in favour of this agent, its importance, and the extent to which it has displaced chloroform, that it has not been the subject, now for many years, of a complete and thorough treatise. Partisanship has come in, perhaps to warp testimony, certainly to influence judgment, and we find disagreement as to facts and varying deductions from those who have presented us with the result of their studies, and to whom we look for our information.

Restricting observation to the more prominent symptoms and the major functions affected during administration, it may be said that in only one point do ether and chloroform present a close similarity of action. This is in regard to the depression of temperature produced by them. It is the result of Dr. Kappeler's careful study, that a fall takes place without exception under both; this with ether varies from .3° to 1.5° C., average .68, and with chloroform from .2° to 1.1°, average .59. Leaving out the cases having fever, and the remarkable correspondence is shown of .53° for chloroform, and .52° for ether.

Irregularity of respiration is common to both, but is much more marked with ether than with chloroform. It begins with ether at an early stage with that sense of suffocation which its vapour produces, and which is one of the most unpleasant and most unbearable of sensations, it is marked during the whole course of administration until the regular snoring of deep insensibility comes on. This marked interference with this important function is closely allied to or consequent on the much greater amount of muscular excitement produced by ether. While violent muscular action is exceptional with chloroform, it is almost constant under ether. In two hundred administrations Dr. Kappeler saw it violent and long-continued in two-thirds of the cases, and in only ten, all but two of whom were children, was it absent or but slight. That this general muscular rigidity with tetanic contraction of the chest-muscles is a period of special danger with chloroform has already been shown, that the danger at this point is not so great under ether we believe certain. Dr. Kappeler saw only three cases in which it became necessary to resort to measures of relief. He admits, in regard to this feature, that much depends upon the manner of administration. Since he has used Clover's lesser apparatus he has found the stage of excitement much less marked.

It is in regard to their effects upon the circulatory system, upon the action of the heart and the blood-pressure in the arteries, that these two agents differ most widely, and the consideration of this difference assumes the highest importance from its close connection with death under their influence. The generally accepted view we believe to be that ether exerts a decided stimulating influence upon the heart and circulation. That it does not exert a depressing influence seems to be the unmistakable result of experiment upon animals. This was plainly shown by the English Chloroform Committee, and it is again shown by the Anæsthetic Committee now pursuing its investigations. We would rely upon any decision reached by that committee with absolute confidence for reasons already given, and the result of this latest and scientific study is that while both chloroform and ethidene administered to animals have a decided effect in reducing the blood pressure, "*ether has no appreciable effect of this kind.*" "*It may be given for an indefinite period without interfering with the action of the heart.*"

But when we turn to investigations as to this point made upon human beings we regret to find that the testimony does not agree. Dr. Turnbull quotes the observations of the late Dr. Morgan of Dublin with the sphygmograph, gives the tracings, and quotes from him that "the most perfect anæsthesia could be invoked under the influence of ether, with an absolute stimulating effect on the circulation." Dr. Kappeler says that "in ether narcosis deep enough for painless operations, an alteration in the pulse-curve does not follow in every case, while in deep chloroform narcosis the pulse curve shows, without exception, characteristic and marked changes." He has made ten careful observations, and gives in his book seven of the tracings:—

> "Of these ten there were three in which no change, or a scarcely perceptible change in the curve, took place, although the patients were deeply anæsthetized, an occurrence not found in one of the twenty-five cases of chloroform narcosis; *in seven of the ten cases the curve of deep ether narcosis differed not at all from that of chloroform narcosis.*"

This brings us to the consideration of the mode by which death under ether takes place. The general impression we believe to be that death never occurs under this anæsthetic with the frightful suddenness that it does under chloroform, that it is always respiratory and not cardiac. The question is of course no trifling one, since if the route of disaster is by the respiratory function the danger is announced, and there is an immense amount of resource for rescue, while if it is by the heart there is no warning, and the patient is generally dead before the first efforts at resuscitation can be put in force. Dr. Turnbull is positive upon this point. He says that ether "always gives warning before it causes the death of the patient." Nevertheless, cases 6 and 8 of his own table, given only a few pages from the above statement, seem to have been as sudden as any under chloroform as far as can be judged from the brief reports. Dr. Kappeler finds four cases out of nine deaths under ether, in which the fatal symptoms arose from the circulatory apparatus as well as from the respiratory, and in which the death was sudden.

It is at once a singular fact, and a matter for regret, that neither of the works under consideration makes any attempt to present the total number of deaths which have occurred under ether. This seems to be a very important matter, when chloroform is under consideration, but of no consequence in regard to the other anæsthetic! Dr. Turnbull's table contains eighteen deaths, of which nine present no extenuating circumstances, and are pure ether deaths, yet the time covered is only from 1873 to 1878. Dr. Kappeler gives thirteen fatal cases, nine immediate and four soon after administration. Of the nine immediate, two cases were certainly and one probably suffocated by ingesta getting into the air-passages, and three underwent severe operations, leaving four deaths due to the anæsthetic. His collection contains no case previous to 1872. There is a long gap between Perrin and Lallemand[1] and these authors which we do not know how to fill, so that our knowledge upon this point is singularly lacking.

It will have been noted that Kappeler gives a class of deaths occurring after the administration is over, and in this respect ether is no more free from reproach than chloroform, indeed is more open to it. He refers to the investigations of Lallemand, Perrin, and Duroy, as showing that "ether is retained a longer time in the organism, and has therefore a decidedly more prolonged operation." The fact is beyond question and an instance was lately testified to us by an eye-witness. The patient was a healthy young man, the operation extirpation of the eyeball. It was performed under ether, had been completed and so far over that all were engaged in examining the specimen, when all at once dangerous symptoms were manifested by the patient, and only most energetic measures succeeded in saving him. This case has never been published, and it may be mentioned as one offset to the point made, and justly made, in regard to the mortality from chloroform, that very many cases have never been made known.

In attempting to estimate the ratio of deaths from chloroform to administrations one factor is, approximatively, known—the number of deaths. But even this is lacking in regard to ether, and figures are no better than guesswork. We miss very much, as to ether, such statistics as those of the armies of different nations, and those prepared by Dr. Richardson from the hospitals of England in regard to chloroform. Nevertheless, the current doctrines of surgery and the practice of the art do not always depend upon numerical statements, and for the relative safety of the two great anæsthetics we must get what we can from figures and then go beyond them. The authors of the *Traité d'Anæsthesie Chirurgicale* attempted a comparison of the two by numbers, and arrived at three deaths from ether during the first fifteen years of its use, and seventy-seven from chloroform during the first fourteen years. But as chloroform for a time almost entirely banished ether for use, no calculation can be thus made. We would prefer to take the first year during which ether was used, which gives one death, and the first year of chloroform, when, we may assume, it was almost exclusively used, and that gave nine deaths. The very attempt, without the number of administrations of each, shows how vain it is. In the absence of figures, such general statements as the one made by Dr. Turnbull are important:—

> "In the city of Philadelphia alone it [ether] has been used, with but one exception, since its introduction in 1846 up to the year 1878 . . . without a single primary death and only one recorded secondary death."

The exact meaning of "with but one exception" is not clear. It is not possible that chloroform has been but once administered in that city within that time, or that it has been used by but one surgeon!

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[1] Traité d'Anæsthesie Chirurgicale, 1863. They give three deaths from 1847 to 1859, yet Dr. Turnbull, p. 32, gives only four deaths as the number up to 1872.

All the general testimony of this kind is decidedly in favour of the far, the very far, greater safety of ether as an anæsthetic as compared with chloroform. The tone of medical journals as they record the frequent deaths from the latter, the current of professional opinion, the abandonment of the one agent for the other by surgeons almost everywhere, show that the lesson has been learned from facts quite as well as from figures.

Dr. Kappeler, in summing up, says:—

> "It is not possible to obtain an insight into the mechanism of death under ether from such a scanty number of observations. Only further experience and numerous new observations can throw light on the subject. And it is by no means apparent from the material before us that ether-death in human beings is essentially different from chloroform-death, or that, as physiologists hold in regard to animals, the fatal result in man always begins by disturbance of respiration, and in every case proceeds from the respiratory system. The assumption of the physiologists (Schiff), therefore, that the surgeon is responsible for death from ether, in contradistinction to chloroform, and that death from ether may always be avoided by precaution, stands, in my opinion, without any foundation whatever. From the experience of the operating table it cannot yet be maintained with absolute positiveness that the administration of ether as an anæsthetic has a considerable less amount of danger than chloroform. We are as little prepared to state in figures the dangers of ether as those of chloroform, since neither the number of deaths from it nor the number of administrations is known, and the few attempts made to state the proportion of deaths to administrations are mostly the product of the bitter contest: ether *versus* chloroform, and the necessary impartiality is lacking. It is well, however, to bear in mind that the late proclamation from Lyons of the absolute safety of ether, and the late disavowal of the same from the same place, brings us *ad absurdum;* that, as already stated, since the resumption of the use of ether in England, deaths have occurred from it in more considerable number, even by careful administration, and that, with the more frequent use of the agent, the number of accidents has increased."

We can by no means assent to the major proposition; cannot understand, indeed, how the author could formulate it from the facts contained in his own book and from the literature of the subject which he has examined. That he does so, indicates, we think, an absence of that impartiality which he so much deplores. If deaths from ether are multiplying so rapidly, he surely should be able to present a greater number than he has, especially from that country where, we are told, it is being more frequently used, and where the enterprise of the medical journals has won his commendation. The truth is, there is no approach in number of deaths from ether to those from chloroform. The proof *is* "based on the broadest foundation of clinical experience" that ether narcosis is not only "considerably," but *very much* less, dangerous than chloroform narcosis. Then we have the testimony of experiment on animals, repeated again and again, and always strong on the side of the relative safety of ether as compared with its rival. When clinical experience and experiment coincide and sustain each other, the question must be considered settled, for there is no higher court of appeal.

But, as with chloroform, other considerations than those of safety alone will influence the use of ether. The difficulties of its administration are not such as to stand much in the surgeon's way. Yet its tardiness of action, especially mentioned by the Anæsthetic Committee, and its unreliability deserve consideration. Turnbull and Kappeler both give cases which could not be brought under its influence. And, again, there is its inflammability, which rules it out of operations by artificial light. Then there is the vomiting, in regard to which, however, we have the widest differences as to facts. Dr. Turnbull states that in his practice it occurred only once in fifty cases. Dr. Kappeler observed it in twenty-five per cent. of his administrations, against fourteen per cent. for chloroform. Impurity of the article used cannot be adduced to explain this, since he has used only ether from the best pharmacies of Lyons. The disagreeable smell and taste of the ether, which persists for hours, sometimes for days, is a great annoyance to patients; and, although the profuse salivation of inhalation may be a trifling matter, the sense of suffocation which attends it is far from being so. To those who would overlook or despise this feature, we suggest a reference to a jury of those taking it, rather than those administering it. It is at least serious enough to have lately been the subject of an editorial in a prominent medical journal,[1] and we believe with the author of that article that its administration without due consideration of this effect is a piece of positive cruelty.

Mixtures, in varying proportions, of chloroform and ether made their appearance at an early period in the history of anæsthesia. They were the natural outgrowth of dissatisfaction with the disadvantages of ether and experience of the dangers of chloroform. Their use received a powerful impulse from the influence of the Chloroform Committee. The report of that Committee impressed the importance of gradual administration and free dilution of chloroform vapour, both of which are insured by mixture of the liquids, and proved by experiments on animals the less depressing influence upon the heart of the combined vapours. That Committee investigated three mixtures: alcohol one part, chloroform two parts, ether three parts; chloroform one part, ether four parts; chloroform one part, ether two parts; and recommended them for further trial, as combining the effects of the two, and the first especially "on account of the uniform blending of the ether and chloroform when combined with alcohol, and probably to the more equable escape of the constituents in vapour." This mixture, generally known as the A. C. E. mixture, should be called "Harley's mixture," as he first proposed and used it.[2] But, notwithstanding the powerful endorsement and recommendation of that Committee, at a time, too, when the profession was saddened and disappointed by the discovery that chloroform was not trustworthy, none of these mixtures ever succeeded in gaining general confidence or passing into general use. In some localities, and by individual surgeons, these, or similar combinations, have been preferred, and have been used sufficiently to furnish quite a large experience. In this country the late Dr. Atlee used the third mixture of the Chloroform Committee, and under its influence performed three hundred ovariotomies, without accident (Turnbull). The mixture of six parts of ether to two of chloroform has been used so generally in Vienna as to be called the "Vienna mixture," and there have been over eight thousand administrations of it without a fatal case (Kappeler). Billroth, in recent times, has used exclusively a mixture, chloroform three parts, ether and alcohol, of each, one part, believing it to be safer than chloroform.

The accidents from these mixtures have not been numerous. Dr. Kappeler gives two, and Dr. Turnbull four, as occurring in "recent times." Undoubtedly there have been others which have never been recorded, but of the above none occurred with the A. C. E. mixture, nor have we been able to find a report of one.

It may be interesting to inquire why these mixtures have not been received with more favour by the profession, since their record is good, and in view of the high recommendation of the Chloroform Committee and of all the theoretical considerations in their favour. We find the explanation in two facts. First, the occurrence of a death at an early day, which is known as the "Crockett case."[1] Now, looking critically at the facts of that case, we boldly say that there is not a more flagrant instance of transfer of the effect of an operation to the anæsthetic than in this instance, and this is saying a good deal! The boy was five years old, a tumour was removed from his back which required for its removal two incisions "each nine inches long," the operation was stopped to ligate "a large artery," five other arteries were tied, the little fellow lost "four ounces of blood, certainly not exceeding six," and then vomited and died! The rapid loss of this amount of blood, the shock of such an operation in so young a patient, pass for insignificant, and the anæsthetic mixture has been paraded as the sole cause of death, and is so given in the latest work on anæsthesia! This is not justice, nor is it science. But that this case has exerted an immense influence in preventing and restraining the use of mixed anæsthetics we most firmly believe.

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[1] Medical Record.

[2] Sansom on Chloroform, p. 138.

The second explanation we adduce is the energy and persistency with which the fact of the unequal rate of evaporation of the various liquids which make these mixtures, has been pushed as an argument against their use. The plea has been that because the more volatile and safer liquids pass off first, the patient will, after a time, get the more powerful and dangerous one alone, when he is supposed to be getting the mixture. It would seem that such an objection is too trifling to have any weight, because it needs but to be stated to be recognized and avoided. The danger is not easily escaped perhaps, if the mixture be given by pouring it on a towel or napkin, the worst way ever devised for the administration of an anæsthetic, because certain to insure uncertainty as to the amount the patient may get at each inspiration, and as to the rate of his progress to unconsciousness; but if given from a sponge, a squeeze now and then, and a fresh addition of liquid, obviates the difficulty at once, and an objection is removed that has been urged, and repeated, and impressed as if it were of equal magnitude with the sudden and irregular action of chloroform.

More recently the strong feeling in the profession against chloroform, has led some who had used these mixtures, to abandon them. An eminent surgeon of the northwest lately stated to the writer that he had long and extensively used the A. C. E. mixture, and had never found cause for dissatisfaction, but that he had given it up solely under the influence of the feeling above alluded to. Our personal experience with this mixture has been such as to enable us to feel the full force of the point made by those who have used chloroform all their lives without accident. By dilution of the chloroform vapour there is no probability of producing death by cardiac paralysis, so prominent a feature of chloroform, while it is prompter in action, more reliable, and less unpleasant, than ether. That it is safer than ether, no one would maintain; that it is less safe remains yet to be shown. Undoubtedly the greater or less frequency of occurrence of dangerous symptoms, could they be ascertained, would be quite as good a test of the safety of an anæsthetic as the number of deaths. In this respect the mixture has been more favourable in our hands than ether in the hands of Dr. Kappeler: he was obliged to resort to measures of resuscitation three times in two hundred administrations. With the A. C. E. mixture we have been obliged to resort to them but once during some thirteen or fourteen years' constant use.

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[1] Am. Journ. Med. Sci., July, 1857.

The modification of the ordinary course of anæsthesia by the preliminary injection of morphia deserves attention. Resorted to first in man by Nussbaum, and its advantages shown about the same time by Bernard by experiments on animals, it has been the subject of numerous articles from time to time since, and has probably been used more in practice than would appear from the literature of the subject. It is claimed for this "mixed narcosis," that it is especially adapted to prolonged operations by rendering a far less quantity of chloroform necessary, the anæsthesia being continued with far less frequent repetition of inhalation, that the stage of excitement, both muscular and mental, is lessened, and that thereby the dangers of anæsthesia are diminished. Mollow, one of its enthusiastic advocates, goes further, and claims that the action of the morphia lessens irritability of the air passages and so restrains reflex action upon the heart, that in this respect its effect is similar to division of the *par vagum;* also that the morphia in small doses, increases the blood-pressure by its action on the motor ganglia of the heart, and by its contraction of the peripheral vessels, thereby opposing the chief deleterious influence of chloroform from the beginning, by presenting an opposition which must be overcome before the vascular pressure can sink below the normal. As to the smaller quantity of chloroform necessary for a given length of anæsthesia, the less amount of muscular excitement and the modified mental condition, Dr. Kappeler says "the advantages have been on various sides clinically proved, and are only seldom called in question."

There have been dissenting voices heard, however; among them, Demarquay, who claimed that the dangers of anæsthesia were by this method in no degree lessened—rather increased by the combination of two such depressing agents. If Mollow's claim as to the action of morphia be correct, of course this falls to the ground. The latest investigators of this method are Heiter and König, who have resorted to it with good results and warmly recommended it. As to danger König sees no difference between chloroform and morphia-chloroform narcosis.

Dr. Kappeler makes a very important contribution to this subject, and gives eleven carefully observed and recorded cases of the simple administration of chloroform and then of the mixed narcosis to the same patient, and almost all of the administrations for the same disease. We cannot omit presenting his conclusions in his own words upon so important a branch of the subject, conclusions drawn, too, from the use of so rare clinical opportunities. He makes the hypodermic injection from twenty to thirty minutes before administering the anæsthetic, and the time is not unimportant, as the stage of excitement is more probably increased than diminished if the one immediately precede the other. He states the amount of muriate of morphia used to have been for adults .015, for children .01.

> "From this table, and a larger number of other chloroform-morphia narcoses, (about 150) it appears that the course of the anæsthetic with morphia is nearly the same as without it, that nevertheless the mixed narcosis is in general of a more peaceful character, that the stage of excitement is shortened, and the patient carried more rapidly and with less muscular action into the stage of tolerance. This difference is particularly marked in subjects more or less under the influence of alcohol. Passing to the individual symptoms it may be said that the reduction of the frequency of the pulse occurs in the mixed narcosis about the same as in the simple. Two cases of the table seem to show that the depressing influence of

chloroform upon the action of the heart manifests itself less when morphia is combined with it; yet I saw lately, in the mixed narcosis, the patient a woman thirty-six years old, very dangerous symptoms on the part of the circulation, weak, irregular, intermittent pulse, with pallor of the face, so that the administration had to be stopped; and in my table of deaths from chloroform, there is a case of fatal syncope in which an injection of morphia preceded the inhalation. The temperature falls with and without morphia. The very frequent irregularity of respiration, amounting often to threatened suffocation, appears decidedly less frequently in the mixed narcosis. The dilatation of the pupils of the early stage of anæsthesia is frequently wanting when morphia is given. Muscular contraction is not present in the same degree as without morphia, and sensibility is abolished sooner and more completely, while vomiting is rather more frequent from the combined method. It is entirely certain that after a hypodermic injection of morphia, a greater effect is produced by the same amount of chloroform than without it; that is, a more rapid and a deeper narcosis is produced. The average smaller consumption of chloroform by mixed narcosis reduces somewhat perhaps the danger of the anæsthesia."

It appears reasonable that this process is especially adapted to one particular class of patients, nervous females or any patient looking forward with great dread either to the operation or the anæsthetic. Such powerful and depressing mental influence is the only explanation of the sudden deaths on the operating table before anæsthetics were known; it has been present in not a few of the deaths under anæsthetics. Such mental action is blunted and subdued by the narcotic, and the patient can then be gently conducted along a path which, without it, he could only travel with resistance and fear, if not horror.

It appears that none of the advantages of chloroform-morphia narcosis attach to ether-morphia narcosis. Dr. Kappeler gives his experience with twenty-five cases, the particulars of which it is not necessary to give, and this seems to show that in all respects the combination of these two agents is rather injurious than beneficial. Nor does an experience of seventy cases of ether-chloral narcosis allow him to speak any better of that combination.

Passing to the consideration of anæsthetics other than, and substitutes for, the two great ones, little time need be devoted to nitrous oxide. Its difficulty of preparation, and impossibility of transportation, unless with costly apparatus, prevent its use anywhere except in hospitals, and it is not adapted for general surgical practice. Long and severe operations, such as ovariotomy, have been performed under its influence, but not in sufficient number to establish its relative greater safety. The promptness of its action, the rapidity with which consciousness returns, and the absence of unpleasant symptoms afterwards admirably adapt it for brief operations, as extraction of teeth, while abundant clinical experience has shown it to be for such purposes safe, as safe probably as any anæsthetic will ever be found to be. Dr. Turnbull gives the statistics of the Colton Dental Association up to 1870 as 75,000 administrations, and those of the Drs. Thomas, of Philadelphia, up to 1879, as 58,400, making a total of 133,400, without a single death, and with not more than three in a thousand exhibiting unpleasant symptoms afterwards. Meanwhile it has its black list. Dr. Turnbull gives four cases, in one of which, however, the post-mortem examination revealed a cork, which had been placed between the teeth, in the larynx. Dr. Kappeler gives three fatal cases, one of which is certainly and the other probably in Dr. Turnbull's list. The third deserves record for some extraordinary features. It occurred to Nussbaum. The patient was a hard drinker upon whom perineal section had been performed. For the passage of a bougie he had taken chloroform *fifty-three times!* the fifty-fourth time he took nitrous oxide and never woke up! Deep cyanosis came on during the administration, and he died notwithstanding respiration continued for fifty minutes, and was maintained fifteen minutes longer by faradization of the phrenic nerves.

Especially adapted as is nitrous oxide for dental operations, and safe as it has been shown to be, the administration of any other anæsthetic by a dentist should be considered criminal.

The bichloride of methylene is scarcely in the field as a surgical anæsthetic. As it has never been manufactured and placed within reach of the profession in this country, there has been almost no experience with it here. Indeed it is difficult to obtain it anywhere pure, different specimens showing varying proportions of other ethers. The Anæsthetic Committee speak of it as "bichloride of methylene, so called, but as it has not a definite and constant boiling point, it is obviously a mixture."

This is well known to be the favourite anæsthetic of Spencer Wells, and his experience, extending to over 350 major operations, 280 of them being ovariotomies,[1] seems to justify him in speaking with great confidence of its merits. This shows conclusively that methylene can be successfully used for the severest operations, yet it proves comparatively nothing as to relative safety, for twice four hundred administrations is but a small number to test an anæsthetic. When it is added that other observers have not found this anæsthetic safe, and that the list of accidents from it is a large one, its use is not likely to be extended. Dr. Kappeler gives the details of nine deaths from it; two of them took place in the Moorfields Ophthalmic Hospital within two years. He details the experience of German surgeons with it, Nussbaum, Hegar, and Kaltenbach and Kocher, none of whom prefer it to other anæsthetics; and his conclusion is that "experience shows it to be as dangerous, if not more dangerous, than chloroform." This, too, is plainly shown by the experiments of the Anæsthetic Committee:

> "With frogs, it was found that the heart became quickly affected and soon stopped. With rabbits, respiration rapidly deteriorated and stopped while the heart was still beating. . . . As in the case of chloroform, the right ventricle became enormously distended—the first sign of paralysis being the commencement of this distension."

If nitrous oxide and bichloride of methylene claim and deserve but little consideration as surgical anæsthetics, two other anæsthetics have recently made their appearance as candidates for professional favour, one of them in England and the other in this country, which are now exciting the deepest interest and attracting the closest attention. The English article is the result of the study of the Anæsthetic Committee for some agent which would be free from the dangers of chloroform. They believe they have found it in the dichloride of ethidene ($C_2H_4Cl_2$, "an isomeride of ethene dichloride produced from aldehyde"). This they have found to yield the best results of all the articles experimented with. The mode and character of the experiments have already been spoken of, and here the results alone concern us. In rabbits anæsthesia was produced in four minutes, and cardiac action continued vigorously for forty minutes. Dogs were anæsthetized in from two to three minutes, and continued half an hour without failure of the heart's action. A most striking contrast was shown by changing the vapour of ethidene to that of chloroform. Under the latter

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[1] Diseases of the Ovaries.

"the right side of the heart began almost immediately to become distended and to be dark in colour, and the activity of the heart rapidly failed."

Ethidene is not free from the charge of reducing the blood-pressure, but it does not do it to the same extent as chloroform, and the reduction is "by regular gradations, and not, so far as observed, by sudden and unexpected depressions."

"Under the use of ethidene there was on no single occasion an absolute cessation either of the heart's action or of respiration, although they were sometimes much reduced."

Vomiting is sometimes produced by it, but whether more frequently than by other anæsthetics has not been determined.

The conclusion of the Committee is that although ethidene is not free from danger, it is in a very high degree safer than chloroform; that it is less exciting, more agreeable than, and as rapid in action as chloroform; and that "a very strong case has been made out for an extensive trial of it."

The experiments upon animals with ethidene seem to be very satisfactory, and the recommendation of this Committee is a strong passport to professional favour. The amount of clinical experience with it has not been large, but to begin at the beginning of this part of the subject we must go back quite a time in the history of anæsthesia. Ethidene was used by Snow as early as 1851, and his work on anæsthetics contains a chapter on it, under the title of the monochlorurretted chloride of ethyle, in which his clinical experience with it is detailed. He administered it for about seventeen operations, some of them of considerable severity, and from what he saw of its action formed a very favourable opinion of its merits. He says of it that "it resembles chloroform very much in taste and smell, and its physiological properties. Its boiling point is 149°; whilst that of chloroform is 140°; the specific gravity of its vapour is also lower; for these reasons it is considerably less volatile than chloroform, and it is therefore pretty certain that it would not be liable to cause the sudden deaths which have occasionally been produced by the administration of chloroform, even if it were given freely, and with no great care."

Difficulty of obtaining ethidene pure prevented Snow from using it to any further extent, and his last illness fell upon him even as he was writing the last paragraph of his chapter upon this article.

The second report of the Anæsthetic Committee is occupied with the detail of six cases in which ethidene was administered in various operations, most of them not severe, but in one, for the radical cure of hernia, the action of the anæsthetic was maintained for twenty-five minutes. In all its action was satisfactory, and the Committee does not hesitate to say that "it presents all the advantages of ether without any of its disadvantages."

Some other clinical experience with this article has been published in the British journals, all of it favourable to the new agent. Yet there is an opposite side to the account, although it has been so recently opened. First, theoretically, Richardson devoted a great deal of study to the different anæsthetics seeking a substitute for chloroform, and one of his statements of results is to the effect that "all the members of the group of narcotic vapours of the chlorine series are dangerous narcotics." This, then, must be disposed of by further study and observation. Second, and practically, Dr. Kappeler closes the page devoted to ethidene, and in which he speaks most favourably of it, with the following paragraph:—

"Unfortunately, the occurrence of a death from this article, in Berlin, destroys early the hopes which had been cherished of it, and will not permit of a more extended trial."

It is deeply to be regretted that no particulars of this case are given, and in the absence of them it is well to suspend judgment. The list of accidents from anæsthetics is full of cases in which the death is most unjustly charged to their influence and in which the vapour played but an insignificant part.

The other new anæsthetic, and the one which is just now receiving so much attention in this country, is of course hydrobromic ether, or the bromide of ethyl (C_2H_5Br). For its introduction into practice here as an anæsthetic, and for the impulse which has been given to its use and the study of it, we are indebted to Dr. Turnbull. The statement in his book that he "was the first to experiment with this ether upon man," we choose to consider as one of those slips of the pen which are constantly occurring to busy men when they write. Mr. Nunnely, of Leeds, England, used bromide of ethyl on animals as early as 1849, and his experience with it as an anæsthetic with patients was the subject of a paper read by him before the British Medical Association in 1865. Dr. Turnbull has been first to use it here, and to urge its good qualities and its adaptation to use for surgical purposes; and for his enterprise he deserves great credit, as he will receive due honour, should the article prove to be all that its friends claim and all that every one must hope it will prove to be,—as efficient, as reliable, as pleasant as chloroform—as safe as ether. Dr. Turnbull has been fortunate in finding a follower and able coadjutor in Dr. Levis, of Philadelphia, whose position has enabled him to give the article a more frequent use and a wider range of operations for trial. The experience of the former is given in his book, and amounts to seventy-one administrations, of which some particulars are detailed. Dr. Levis states his experience in a more general way, but no testimony could be stronger in its favour. He has "used it under the most varied circumstances which could be required to test the merits of an anæsthetic, . . . and in the most abnormal conditions of debility and shock of injury, in capital operations, through protracted periods of administration, in patients from early infancy to extreme old age," and has been so satisfied with its effects that he is impressed with the "conviction that it is practically the best anæsthetic known to the profession."[1]

The two leading peculiarities of bromide of ethyl are quickness of action and speedy recovery from the anæsthetic condition. The shortest time stated by Dr. Turnbull, in which anæsthesia was produced, was thirty seconds. Personal experience showed consciousness lost with three full, slow inspirations, and complete anæsthesia produced in a minute and a half, while consciousness returned in the same length of time.[2] Pleasantness of effect must also be given as a decided point in its favour; it is pleasanter to inhale than chloroform. As to the frequency with which vomiting attends its use, of Dr. Turnbull's twenty-one cases, in three this symptom followed, which is about fourteen per cent. Dr. Levis states that it occurs less frequently than after ether and chloroform. The state of excitement or struggling is stated by one of these gentlemen to be "short," by the other, "more moderate, brief, and transient than in the early stage of the anæsthesia of ether and chloroform." But upon this point the mode of administration has a powerful influence; mixed with air, as is the case with ordinary ether, the stage of excitement is violent and prolonged;[1] given pure and rapidly the above statements are probably correct.

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[1] Ethylization: Reprint from Medical Record.
[2] Cincinnati Lancet and Clinic, April 10, 1880.

The behaviour of the great functions of circulation and respiration is, of course, the great point in the study of a new anæsthetic, and in regard to these, under hydrobromic ether, we are sadly lacking in accurate information. Dr. Levis has not given any detailed account of administrations. He states, in general terms, "that the circulation generally shows evidences of moderate excitement, as indicated by some increase in the rapidity of action of the heart, and the pulse evinces greater general arterial tension." "The respiration is slightly increased in frequency until anæsthesia becomes complete, when it assumes the character of normal sleep." Of the fifteen cases given by Dr. Turnbull, with more or less detail, in nine no mention is made of these important functions, in one other the pulse is not given; while respiration is stated as not interfered with; in another, the pulse was "slightly accelerated," and the respiration is not mentioned! This leaves only four cases in which particulars of so great importance are given in a study of such a character as this! In one of these the pulse "was not influenced in the least," and the respiration was "slightly accelerated." In case 11 the pulse was, before the administration, 100, after first five inspirations 160, after operation 120. In case 13 the pulse was, before, 100, during the first eight minutes it was 160; the respiration 28, at sixth minute 30. In case 15 the pulse "advanced to 84," the respiration was "not interrupted." Cases 11 and 13 are notable for the enormous increase of the pulse rate, and they correspond with our personal experience, in every instance the pulse mounting in the first minute from about 80 to 120 and rapidly falling. It is singular that so marked a feature, occurring with certainty twice out of fifteen times, should not have occurred to Dr. Levis. How much we miss here the careful observation and minute record of individual symptoms of Dr. Kappeler!

This brings us to the consideration of an unfortunate case reported by Dr. Sims,[2] the only fatal case which has occurred after the administration of bromide of ethyl, a case which has been so prominently presented to the profession, and which has attracted so much attention, that no restatement of its particulars can be necessary. Only those need be looked at which bear upon the question of the safety or danger of hydrobromic ether. First, is the marked effect upon the respiration; it was very rapid, the patient breathed "sixty times a minute." In the debate in the New York Academy of Medicine, which followed the report of the case, another was mentioned (Dr. Little's second), in which "both the pulse and the respiration became exceedingly rapid." It is evident that Dr. Sims's patient did not die from any cardiac influence of the anæsthetic, and this is a point strong in its favour. The operation lasted an hour and a half, during all which time "her condition was good and her pulse was strong and full." We think it must be conceded that she did not die from shock or from any direct influence of the operation as the major factor; the time at which death took place, twenty-one hours after the operation, would militate against this, but more especially the marked symptoms present, of vomiting, pain in the head, and choleraic diarrhœa. She did not certainly die as she would have died from any other anæsthetic. A peculiar feature of the case was the scanty secretion of urine—five ounces and one drachm in twenty-one hours. As the kidneys, to the eye, appeared normal, and, under the microscope, revealed "acute catarrhal nephritis," the question arises whether this scanty secretion was an effect of the ether, of the condition of the kidneys, or an accidental occurrence.[1] Future observation and experimental research can alone decide. Many years ago, Dr. Emmet called the attention of the profession to the active part taken by the kidneys in the elimination of ether from the system, to the necessity of examining the urine before administering an anæsthetic, and to the great danger when disease of the kidneys exists.[2]

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[1] See Dr. Sims's first, and Dr. Little's second case, Med. Record, April 3, and Cincinnati Clinic, April 10, 1880.

[2] Medical Record, April 3.

Adding the general statement that hydrobromic ether has been administered with safety about two hundred times, and clinical experience is exhausted. Looked at carefully, it does not seem to justify the enthusiastic confidence in the safety of the agent which some of its friends show. The violent perturbations of the great vital functions of respiration and circulation would seem to enforce distrust of it; and that sometimes one and sometimes the other is affected looks like that irregularity of action and inconsistency of course which have banished chloroform from practice. The rise of the pulse rate from 80 to 120, or even to 160, within a minute, certainly suggests doubts as to a heart, at all diseased, bearing the strain.[3]

Theoretical considerations come up for examination, and are especially necessary so long as clinical experience is lacking. These are chiefly, or entirely chemical. If the dictum of Richardson in regard to the chlorine compounds be true, why would it not hold good for the bromine series? If we disintegrate compounds, and find them safe, in proportion to the innocuousness of their components, and *vice versa*, then an agent, into the composition of which bromine largely enters, can stand in no better, if in as good a position, as one containing chlorine. This was the line of argument pursued in the debate on Dr. Sims's case by Dr. Squibb, and its force is not easily escaped, while the facts he presented then, in regard to the chemical peculiarities of bromide of ethyl, its easy decomposition, and its loose molecular character, cannot but bear with very considerable weight in deciding the question.

Dr. Squibb acknowledges that he is prejudiced against hydrobromic ether. This feeling may influence his opinion, and give tone to his remarks, but cannot affect chemical facts. Yet a fuller and more accurate account of the chemistry of this article is now in order, is, indeed, a necessity, and the profession must look to chemists for it. But what is more urgently needed is a series of experiments upon animals. Those given in Dr. Turnbull's work are meagre beyond description, considering the importance of the subject, occupying less than fifteen lines. We want experiments conducted in the scientific spirit, and with the care and accuracy of the British Anæsthetic Committee. Respiration and circulation should be separated, as in those experiments, and then we shall be enabled to judge how frequently and to what extent each is affected by the inhalation of bromide of ethyl, and in what direction danger lies. Then further use of it with patients will have a scientific basis which it has not now, and until then, we do not think surgeons will be likely to use it for prolonged operations with Dr. Sims's case fresh in memory. For operations of brief duration the clinical testimony in its favour seems to fully justify its use.

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[1] *Scanty urine* was a prominent symptom after the administration of hydrobromic ether in a case reported by Dr. Donald Maclean, of Michigan, in Medical Record, April 24, 1880.

[2] Gynæcology, 2d ed. p. 746.

[3] [Since this almost prophetic paragraph was penned, a case of death while under bromide of ethyl has occurred in Dr. Levis' Clinic at Jefferson College Hospital. See current number of *Medical News and Abstract*.—ED.]

Our task has been executed during a period of greatly renewed interest in and study of anæsthetics; in regard to some the ink has scarcely dried before new facts were presented for consideration or statements had to be modified. This awakened interest in the subject and this rapid investigation of new agents attest at once the great importance of the subject and the earnest desire of the profession for a pleasant anæsthetic safer than any we now have. Such an one is not yet found. Every one must deeply regret that a death should have occurred to shake confidence in an article which promised a realization of our hopes. Yet when we reflect on the number of successful inhalations necessary to prove that a new anæsthetic is better than those already tried, the difficulty of the problem is appreciated; the new candidate must have been used thousands of times before it can rival chloroform or the mixed vapours, and tens of thousands before it can be compared with ether in regard to safety. Were this fact kept in view, there would be fewer journal articles and less dogmatism upon this class of remedies. Is it probable that a perfectly safe anæsthetic will ever be at our command? The time necessary for the proving of new agents makes this very improbable during the lifetime of those who saw the advent of ether and chloroform, and the retrospect we have made is not favourable to an affirmative answer. Disaster has followed the use of all, and the facts now will sustain Velpeau's statement as well as when he made it, and still justify Erichsen's words: "With every possible care it appears certain that the inhalation of any anæsthetic agent is in some cases almost inevitably fatal." The inference is that what has been so universally true in the past will be true in the future, however sad this may be to the philanthropist or to the enthusiastic surgeon. Unfortunately physiology teaches the same lesson: a state of artificial anæsthesia is a state on the borders of death.

The great practical lesson of all facts and all theory is that in the use of anæsthetics no precaution can be superfluous, no care too minute, and no watchfulness too great.

J. C. R.

ART. XVI.—*Surgery in the Pennsylvania Hospital. Being an epitome of the practice of the Hospital since* 1756; *including collations from the surgical notes, and an account of the more interesting cases from* 1873 *to* 1878; *with some statistical tables.* By THOS. G. MORTON, M.D., and WILLIAM HUNT, M.D., Surgeons to the Hospital. *With papers*, by JOHN B. ROBERTS, M.D., and FRANK WOODBURY, M.D., late Resident Physicians in the Hospital. Prepared by direction of the Managers of the Hospital. 8vo. pp. 349. Philadelphia: J. B. Lippincott & Co., 1880.

"A HOSPITAL fulfilling its true mission is at one and the same time a house of mercy for the helpless and a school of science on which even the richest are dependent."

Has the Pennsylvania Hospital in its more than century long life ful-